

ON THE

(16)

FŒTUS IN UTERO,

AS

INOCULATING THE MATERNAL WITH THE PECULIARITIES
OF THE PATERNAL ORGANISM;

AND ON

MENTAL STATES IN EITHER PARENT, AS INFLUENCING
THE NUTRITION AND DEVELOPMENT OF
THE OFFSPRING.

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ON THE FŒTUS IN UTERO, ETC.

IN a paper published last year in the "Edinburgh Monthly Journal of Medical Science," I brought together a considerable number of instances, which serve clearly to show, that the peculiarities of a male animal that has once had fruitful intercourse with a female, can sometimes be more or less distinctly recognised in the offspring of subsequent connections of that female with other males. I therein stated, that of this singular phenomenon two explanations used formerly to be offered,—one class of physiologists referring it to a permanent impression made somehow by the semen of the first male on the ova of the female,—another class ascribing it to an abiding influence exerted by that male on the imagination of the female, and recurring to, and operating on, her mind at the time of her connection, subsequently, with other males. At the same time, I expressed an opinion, that neither of these explanations could be regarded as satisfactory; and I endeavoured to show that an explanation offered by Mr M'Gillivray of Huntly, is the true one,—viz. that while, as all allow, a portion of the mother's blood is continually passing by absorption (and assimilation) into the body of the foetus, in order to its nutrition and development, a portion of the blood of the foetus is as constantly passing, in like manner, into the body of the mother;—that as this commingles there with the general mass of the mother's own blood, it inoculates her system with the constitutional qualities of the foetus;—and that, as these qualities are in part derived to the foetus from its male progenitor, the peculiarities of the latter are thereby so engrafted on the system of the female, as to be communicable by her to any offspring she may subsequently have by other males.

Since the publication of that paper, I have learnt nothing to shake, but much to support, the notion of *inoculation* therein suggested; and

have ascertained that many among the agricultural body in this district are familiar, to a degree that is annoying to them, with the facts then adduced in illustration of it,—finding that, after breeding crosses, their cows, though served with bulls of their own breed, yield crosses still, or rather mongrels; nay, that they were already impressed with the idea of *contamination of blood* as the cause of the phenomenon,—possibly, however, on the principle (adverted to by D'Alembert), that the doctrine so intuitively commended itself to their minds as soon as stated, that they fancied they were told nothing but what they knew before,—so just is the observation, that “truth proposed is much more easily *perceived* than without such proposal it is *discovered*.¹

But, with regard to any one of the cases formerly given, as instances of inoculation by the foetus, I would not now assert that it is an *unequivocal* instance; nor would I now affirm that the notion entertained by Sir Everard Home and others, of all of them being an affair of the imagination, is so absurd as it once seemed to me. The careful consideration, recently, of cases seemingly identical in kind with these, save and except that there never was coitus between the mother of the animals and the male animal which they resembled, has convinced me that it is not so clear a matter that they may not be referable to the agency of mental causes. A setter bitch, for example, takes a fancy to a cur dog, and without ever having access to him, produces once and again, to dogs of her own breed, whelps bearing a decided resemblance to the cur. I am still inclined to think, however, that there are good grounds for believing that the greater number at least of the cases are truly referable to the principle of inoculation; and it is very clear that such facts as those exemplified in the progeny of the setter are noways adverse to that theory.

It seems to me probable, indeed, that future inquiry will demonstrate, that there are in nature two sets of cases connected with the peculiarities of offspring, analogous, nay, identical, in their external characters, yet essentially different in their origin and cause,—the one referable to mental states in one or other of the parents, and oftenest in the female parent,—the other to a change effected in the constitutional powers of the female by a physical (organic) agency originally extrinsic to her, but inherent in a former foetus by derivation from its male progenitor, and conveyed to her, in the way of inoculation, by that foetus while in utero.

To establish this, and to determine also the relative proportion of the two sets of cases, together with the comparative efficacy of their respective causes, and the conditions that are essential to the agency of each cause, would be an achievement in physiology of great general interest, and, to breeders, of much practical value. It were idle, perhaps, to engage in so wide and difficult an inquiry with any hope

¹ Archbishop Secker, Works (Ed. 1811), vol. iii. p. 237.

of complete success. But as to some important points, and, in particular, as to the main question here at issue, experiments in breeding on the lower animals, so conducted as to be free from essential sources of fallacy, might, I am informed, easily be made ; and as they could be carried on on the large scale, decisive results might ere long be obtained. Some suggestions as to the kind of experiments which might be instituted, and the mode of conducting them, will be offered in the sequel. As to their probable issue, it were, perhaps, wise at present not to hazard an opinion. But, "if reason can sometimes go farther than imagination can venture to follow," the latter can sometimes go in advance of, and pioneer the way to, the former. And possibly in this instance, if leaving *reason* to follow at her leisure in the broad day and clear sunshine, we hold on our way, warily, in the dim twilight, in company with *imagination*, we may, under her guidance, securely reach a post in advance, and have the satisfaction afterwards of finding reason treading in our steps.¹

Before entering on the consideration of mental states in either parent, as influencing the nutrition and development of the foetus in such a manner as to cause the latter to resemble a male animal not its progenitor, I purpose first, in this second communication, to submit some additional observations on the theory of inoculation, with the view of placing this doctrine on a firmer footing.

I. (1.) Without at present raising any question as to the nature of the phenomenon, I may remark generally, that, since the publication of my former paper, farther inquiry has satisfied me of the truth of an observation there quoted from Mr M'Gillivray,—viz., that cases of the kind given as instances of inoculation of the maternal system by a former foetus, are, as regards *cattle* and *dogs*, "of every day occurrence ;" and judging from the two remarkable cases presently to be given of the like phenomenon in the *sheep*, they are probably equally common in other classes of animals. I have not, indeed, seen any such myself, but opportunities for doing so have not come in my way ; and the information I have received is only of a general kind, sufficient to excite to farther inquiry, but wanting in specific details. There is one fact, however, kindly communicated to me by Sir John S. Forbes of Pitsligo, which, as perhaps bearing on this subject, it seems proper to mention. It is the fact, observable within these few years at all our great cattle shows, that among the high-bred heifers

¹ "If hypotheses are introduced sparingly, and the grounds on which they rest fairly stated, they are admitted to be part of the process by which the knowledge of the truth is attained, even in the most strictly inductive sciences ; and those who profess to reject and despise them, are not those whose opinions are the most exempt from their influence."—Alison, Preface to *Outlines of Physiology*. 3d Edition.

and cows of the Angus polled breed, *fat is laid on in lumps about the tail*, by feeding of the most moderate description—a feature heretofore considered to be characteristic of the Teeswater breed, and which, as occurring in the breed in question, though usually attributed to improved management and high breeding, may, perhaps, have its true solution in Mr M'Gillivray's theory,—on the supposition that the mother, however pure her own original breed and that of the father, had previously borne offspring to a Teeswater bull.

The following are the cases illustrative of the effect on the procreative powers of the *ewe* of previous sexual intercourse with a ram of a different breed; the one communicated to me by my friend Dr William Wells, of the island of Grenada; the other, by Mr William M'Combie, Tillyfour, in Aberdeenshire.

1. A small flock of ewes, belonging to Dr Wells, were tupped a few years ago by a ram procured for that purpose from the manager of a neighbouring estate. The ewes were all of them *white* and *woolly*. The ram was of quite another breed, being (besides having other marks of difference) of a *chocolate* colour, and *hairy* like the goat. The progeny were of course crosses, bearing, however, a great resemblance to the male parent.

The next season Dr Wells procured another ram of precisely the same breed as his ewes. The progeny of this second connection showed distinct marks of resemblance to the former ram in *colour* and *covering*. And the like phenomenon, occurring under the like circumstances, was noticed in the lambs of some other adjoining estates in Grenada, and was the occasion of equal surprise and perplexity to the owners of the animals.

2. Six very superior pure-bred *black-faced horned* ewes the property of Mr Harry Shaw, in the parish of Leochel-Cushnie, in Aberdeenshire, were tupped in the autumn of 1844, some of them by a Leicester, *i.e.*, a *white-faced and polled* ram; others of them by a Southdown, *i.e.*, a *dun-faced and polled* ram. The lambs thus begotten were crosses.

In the autumn of 1845 the same ewes were tupped by a very fine pure black-faced horned ram, *i.e.*, one of exactly the same breed as the ewes themselves. To Mr Shaw's astonishment, the lambs were all without exception *polled* and *brownish* in the face, instead of being black-faced and horned.

In autumn 1846 the ewes were again served with another very superior ram of their own breed. Again the lambs were mongrels. They did not, indeed, exhibit so much of the characters of the Leicester and Southdown breeds, as did the lambs of the previous year; but two of them were *polled*, one *dun-faced*, with *very small horns*, and the other three, *white-faced*, with *small round horns only*. Mr Shaw at length parted with those fine ewes, without obtaining from them one pure-bred lamb.

(2.) Of the analogous phenomenon, in any of the aspects formerly specified, in the human species, I have procured no additional examples. Inquiries are on foot, in one or two of the West India Islands, as to whether the offspring of negro parents, after the female has had children by the European, ever exhibit traces of the latter; but the results have not yet reached me. Dr Maunsell, of Dublin, however, has mentioned to me a case, bearing on the communication of secondary syphilis,—in too imperfect a form, indeed, to warrant its being adduced as an instance of the kind in question, but valuable as justifying the supposition formerly advanced, that a woman may

somehow acquire syphilis, but have it in the *latent* form, and subsequently give proof of the reality of the fact by the birth of a syphilitic child, got by a perfectly healthy man. It is the case of a woman, who, as far as Dr Maunsell could learn, never herself exhibited any signs of syphilis, yet produced a syphilitic child in a second marriage, with a husband who never had the disease.¹

With regard to the communication of secondary syphilis, in relation to Mr M'Gillivray's theory, Mr Paget, of St Bartholomew's Hospital, makes an important suggestion. "I would venture to suggest," Mr Paget writes me, "that you should try to find whether ever a woman derives secondary syphilis from her husband, *unless she conceives by him*. Facts bearing on this point might prove, that secondary syphilis is not communicated directly by the seminal fluid, but by the child begotten with it; and this mode of inoculation being proved would go far to prove the foundation of your [Mr M'Gillivray's] theory." I fear it will turn out, on inquiry, that secondary syphilis may be transmitted directly by the seminal fluid, independently of conception; but perhaps it may appear also, that its transmission in this way is *occasional* only and *uncertain*, while it is *very frequent*, or almost *inevitable*, when conception follows intercourse. And a comparative observation of this kind, if clear and undoubted, would be nearly equally decisive.²

¹ Since the publication of my former paper, I find that Dr Montgomery of Dublin has been beforehand with me in this question as to syphilis; and that he seems virtually, though obscurely, to enunciate the doctrine of the *constitutional* character of the phenomenon exemplified in Lord Morton's mare and Mr Western's breed of pigs. Referring to these well-known cases, Dr Montgomery remarks:—"Such occurrences appear forcibly to suggest a question, the correct solution of which would be of immense importance in the history and treatment of disease. Is it possible that a morbid taint, such as that of syphilis, for instance, having been once communicated to the system of the female [by a conception], may influence several ova, and so continue to manifest itself in the offspring of subsequent conceptions, when impregnation has been effected by a perfectly healthy man, and the system of the mother appearing to be at the time, and for a considerable period previously, quite free from the disease? My belief is certainly in favour of the affirmative."—*Exposition of the Signs and Symptoms of Pregnancy*, p. 18.

² Important as is Mr Paget's suggestion, it may be difficult successfully to follow it out. It appears, indeed, that of the children born syphilitic in the middle and higher classes of society, a very large proportion derive the virus from the father—a circumstance in itself favourable for the prosecution of the inquiry (see Brit. and For. Med. Chir. Rev., No. XII., p. 348). But in several such cases the mother never exhibits any manifest indication of the virus in her own person (Op. cit., p. 347—Maunsell and Evanson on Diseases of Children, 5th edit., p. 452); and although she may not have imbibed the poison, the ease given in the text on the authority of Dr Maunsell is sufficient to show, that the only certain criterion of her immunity may be her bearing a non-syphilitic child in a second marriage with a perfectly healthy husband—a test which can be available only in a very few cases. If, therefore, such cases of latent syphilis in the female are common, Mr Paget's inquiry may fail of an affirmative result only from inability to test them. Possibly, however, the instances of developed syphilis in the female, consequent on conception, may be numerous enough, and sufficiently decided, to lead to that result.

(3.) With regard to the singular observation made by the Count de Strzelecki, and referred to in the appendix to my former paper,—and from which it appears that the aboriginal female of certain countries, after fruitful intercourse with the European, becomes sterile to the male of her own race, though retaining her fertility with the European,—the Count informs me, *that he has never met with an exceptional case,—i. e.*, to use his own words, “it has not come under his cognizance to see or hear of a native female, which, having a child with an European, had afterwards any offspring with a male of her own race.” And I am assured by Professor Goodsir and Dr Carmichael, of Edinburgh, and by Dr Maunsell, of Dublin, that they have learnt, from independent sources, that, as regards the aborigines of Australia, Strzelecki’s statement is unquestionable, and must be regarded as the expression of a law of nature.¹ If it is such, the fact still seems to me to indicate rather a constitutional than a local affection of the female system, and to fall within the compass of Mr M’Gillivray’s theory; to be referable to some influence exerted by the European blood, through the medium of the foetus in utero, on the general system of the female; and to be analogous in its degree to the constitutional effect of the vaccine virus, and of the specific poisons producing small-pox, scarlatina, measles, typhus, &c.,—the one rendering the system incapable, as a general fact, of being again affected by those poisons; the other incapacitating the ova from being again fertilized by the native semen.

Assuming that the fact is truly a law of nature, and that it holds as absolutely and extensively as Strzelecki’s experience would lead us to infer, “it is,” as Professor Goodsir observes, “a very remarkable one, and indicates a series of influences of high import in the natural history of the human race.” What that import is, it may not be easy to comprehend, nor perhaps is it a short line that will fathom it. But it seems to indicate, how little account soever might be taken of it by the author of “*The Vestiges of Creation*,” that there is in nature a principle of *degradation*, as well as a principle of *development*. And it may not be altogether out of place here to remark, that it may form part of the plan of Providence that certain races of men should hold given portions of the earth’s surface till certain other races, and in particular our own Anglo-Saxon race, are ready to step in and occupy them,—those primitive races then disappearing; and that the law in question may be directly subservient to the extermination of these. The rapid increase of the

¹ “The intercourse of Kamehama’s men [people] and that of the whale ship’s [manned by Europeans], which now began to anchor in their waters, was sadly disastrous to the native constitution and morals, poisoning the fountains of health, and inducing premature decay and *barrenness*.” This observation, though by no means very definite, seems to point to the fact alluded to in the text. See an article, entitled “On the Sandwich or Hawaiian Islands,” in the “New York Biblical Repository and Critical Review,” for July 1849.

Anglo-Saxon race during the last two centuries, its wide diffusion over the globe, and its superiority over every race with which it has come in contact, are in harmony with that supposition. It has recently been stated, with regard to this race, that while in 1620 it numbered only about six millions, and was almost exclusively confined to the United Kingdom, it now numbers sixty millions of human beings, planted upon all the islands and continents of the earth, and fast absorbing or displacing all the sluggish or barbarous tribes on the continents of America, Africa, Asia, and the islands of the sea; and that, increasing everywhere by an intense ratio of progression, it is estimated that, if no physical revolution supervene to check its propagation, it will number eight hundred millions of human beings in less than 150 years from the present time.¹

It seems tolerably certain that Strzelecki's law does not extend to the Negro race—the fertility of the Negro female with the male of her own race not being apparently impaired by previous fruitful intercourse with the European male,—a kind of intercourse which is notoriously common in all the West India Islands, the Brazils, and the slave-holding States of North America. And for this immunity the reason may be, a designed appropriation of the Negro race to the cultivation of those regions of the earth, whether in subserviency to the European or independently of him, where, by reason of the high temperature, the European cannot toil in the way or to the degree which that cultivation requires, but where also, but for that provision, the intercourse between the two races might ultimately end in the extinction of the Negro. But it is yet, I apprehend, undetermined, and it must surely be interesting to ascertain whether the law applies to the Mongolian and pure Malay races, inhabiting China, Japan, Borneo, and the islands of the Eastern Archipelago. And, if it should, one cannot help considering, in connection with it, the footing which Britain has of late acquired in those regions,—the probable rapid increase of our countrymen there and in Australia, Van Diemen's Land, and New Zealand,—and the Anglo-Saxon *nation* rising up as with mushroom growth in California, to overspread the western coast of America,—and asking whether, through the instrumentality of that law, the native inhabitants of those parts of the East may not yet be exterminated?

But our knowledge of this law must be far wider and more precise than it yet is, to enable us confidently to speculate in regard to it.² We do not even know for certain that it holds absolutely over

¹ American Paper. Quoted from "Chambers' Edinburgh Journal," for July 1850.

² In arguing, now as formerly, from Strzelecki's inference, it is only provisionally, and on the supposition of its being a fact; and if a fact, then, of course, a *law* of nature;—and in this, as in every other part of these papers, my object is to suggest and direct inquiry, not to dogmatise. But a corres-

the races specified by Strzelecki, or that the Negro race is wholly exempt from it. It is conceivable, in regard to the former, as Strzelecki himself frankly allows to me, that there may be exceptions to it, though he has met with none; and, in regard to the latter, that it may be in some degree subject to it. It is well known, indeed, that in the West India islands, under the system of slavery at least, the black population tends rather to diminish than increase, and that it can only be adequately maintained by continual importations from Africa. Whether that diminution is in any degree owing to the operation of Strzelecki's law, it is perhaps at present impossible to say.¹

(4.) In my former paper, I represent Mr M'Gillivray as holding that there is a direct vascular connection, and a continuous interchange of blood, between the foetus in utero and its mother, and also as regarding that sort of connection to be essential to the validity of his theory of inoculation. Mr M'Gillivray, however, as I have since discovered, does not hold any such opinion, nor does he rest his theory on so insecure a basis as I fancied. His ideas regarding the structure of the placenta, and the connection between the mother and foetus, appear, in fact, to be substantially those suggested by the late Dr John Reid and others, and now commonly received.

The mistake committed by me arose from some expressions dropped by Mr M'Gillivray in the course of his paper, taken in connection with a case given by him, and showing an *empty* state of the blood-vessels of a foetal calf, whose mother had died of a hemorrhage from the lungs, of three days' continuance. As this case seems to me to afford a stronger support to his theory than I at first imagined, and to be besides (though not unique²) a valuable contribution to our

ponent almost laughs me to scorn for giving heed to that inference. On the other hand, a writer in the "Edinburgh Review" (No. CLXXXIV., p. 456, foot note), referring to my former paper, suggests that the excellent and intelligent Strzelecki should "excuse" me for treating it as nothing more than an hypothesis for the present! Considering, however, on the one hand, the astounding nature of the inference, and, on the other, the high character of Strzelecki as a philosophic observer, as well as the extent of his observations (some *hundred* instances of the phenomenon without a single exception), I humbly submit that, until the inference shall be either fully established or shown to be fallacious, the proper course in regard to it is, "to keep the mean between the two extremes of too much stiffness in refusing, and of too much easiness in admitting" it,—or (in the words of the wise and good Bishop Butler) to keep "in the middle state of mind," "between a full satisfaction of the truth of it, and a satisfaction of the contrary"—this middle state of mind consisting "in a serious apprehension that it may be true, joined with doubt whether it be so."—(*Analogy*, Part II., Conclusion.)

¹ It may be mentioned, in connection with this, that it has been stated to me, though not on sufficient authority, in regard to the *mare*, that, though not rendered sterile to the horse, she does not readily conceive by him after breeding with the ass, with whom she continues very fertile.

² There is a case, by Mery, in the "Mémoires de l'Académie Royale des Sciences," 1708. But this case is too briefly recorded to be of any physiological

physiological knowledge, I shall introduce it here, and take occasion to offer some remarks upon it :—

A cow in calf, and past the ordinary period of utero-gestation, had, from some unknown cause, a slight rupture of the left lung. A constant hemorrhage from the fissure proved fatal in three days. On a post-mortem examination, it was found that the hemorrhage had taken place partly into the cavity of the chest, and partly into the air-passages of the lung. A great part of the blood that entered the lung had found its way upwards, and, being swallowed by the animal, was passed along with the egesta. The animal survived until but a very small quantity of blood remained in the carcass.

The uterus, with its contents, was removed entire, and very carefully and minutely examined by Mr M'Gillivray. All the vessels of the chorion, amnion, &c., were white, flaccid, and empty. On making a section of the umbilical cord, no blood followed ; on applying a sliding squeeze on that portion of the cord, and in a direction from the foetus, slight traces of blood appeared at the cut ends of the umbilical arteries, but there was no flow, not even drops. There was no blood in any part of the aorta or vena cava ; none in the carotids or jugulars near the head ; none in the external or internal iliac arteries, and large veins in the ilio-femoral region ; none in the left ventricle of the heart—the right containing a small coagulum which might amount to about half an ounce of blood. In short, there were not three ounces of blood in the calf, taken with all its membranes and placenta.¹

On the perusal, formerly, of this case, I made but little account of it ; and being impressed with the unqualified allegation of various physiologists, that, “when a pregnant animal dies of hemorrhage, the vessels of the foetus remain full of blood,”² I too hastily concluded that some fallacy must attach to it, or at least that, however it might be explained, it was too slender a foundation whereon to rest the doctrine (erroneously assumed as that intended) of a direct vascular communication.

Mr M'Gillivray's real object in adducing the case, was to combat the opinion entertained by some physiologists, that, while the foetus receives supplies from the mother through the placenta, it returns nothing from its own system to her's. “I am quite aware (he says) that many physiologists maintain that, in the highest species of ani-

value whatever. It is as follows :—“Une femme grosse, qui touchait à son terme, se tue d'une chute très rude presque sur le champ. On lui trouve 7 à 8 pintes de sang dans la cavité du ventre, et tous ses vaisseaux sanguins entièrement épuisés. Son enfant était mort, mais sans aucune apparence de blessure, et tous ses vaisseaux étaient vides de sang aussi bien que ceux de la mère. Le corps du placenta était encore attaché à toute la surface interieure de la matrice, où il n'y avait aucun sang extravasé.”—(Op. cit., p. 37.) Very possibly, in this case, as suggested by my friend Dr J. M. Duncan, of Edinburgh, the womb may have been ruptured on its external aspect over the centre of the placenta, and so deeply as to lay open both the foetal and uterine vessels,—in which event the bloodless condition of the foetus would be referable, not to the passage of its blood by *absorption* into the maternal vessels, but to the direct escape of the blood from its own vessels.

¹ Abridged from Mr M'Gillivray's paper in “Aberdeen Journal,” for March 21, 1849.

² Magendie ; Physiology, by Milligan, 2d edit. p. 509. Dr D. Williams, Edin. Med. and Surg. Journal, vol. xxv. p. 102.

mals, the blood cannot be returned from the foetus to the mother during utero-gestation." That this opinion is very generally held by physiologists in this country, is quite certain. Dr Alison, for instance, after observing (on the authority of Magendie and of Dr David Williams, of Liverpool,) that camphor and oil injected into the blood of pregnant animals are soon detected in the blood of the foetus; but that poison, injected into the umbilical arteries, although mixing with the blood on its way from the foetus to the placenta, does not affect the mother; and that fatal hemorrhage in the mother does not apparently diminish the fulness of the vessels of the foetus,—adds, "so that it would seem that the transmission of fluids is almost entirely from the mother to the foetus."¹ Again, Dr Kirkes, referring to Professor Goodsir's observations as to the intervention of two distinct layers of nucleated cells between the foetal and maternal portions of the placenta, speaks of the one being "probably designed to separate from the blood of the parent the materials destined for the blood of the foetus," while the other "probably serves for the absorption of the material secreted by the other set of cells, and for its conveyance into the blood-vessels of the foetus,"²—no idea, seemingly, being entertained of a converse process. Moreover, the view taken by most physiologists of the destination of that portion of the foetal blood which is transmitted to the placenta, appears to be exclusively that of *renovation* or *aeration*, by coming into relation with the oxygenated blood of the mother,³—nothing being said as to *re-absorption* into the maternal system.⁴

¹ Outlines of Physiology, 3d edit. p. 426.—In his History of Medicine, Dr Alison expresses himself even more strongly on the subject:—"The experiments of Magendie and others have proved that any substance which may be circulating in the blood of the mother finds ready access to that of the foetus, but that there is little or no transference of fluids in the opposite direction."—Cyc. of Pract. Med., vol. i. p. lxxxiii.

² Handbook of Physiology, p. 643.

³ Carpenter, Principles of Human Physiology, 2d edit. p. 718. Manual of Physiology, p. 474.

⁴ It may be asked, whether the idea expressed by the terms "renovation" and "aeration" does not necessarily include that of the transference of *some kind* of matter from the foetus to the mother? Supposing "that the umbilical arteries terminate in the umbilical veins, and not in the vessels of the uterus, and that the [whole] blood in the umbilical arteries 'passes from the arteries into the veins, as in other parts of the body, and so back again into the child'"—(Dr J. Reid, *Researches*, p. 318)—still this blood is believed to have acquired something *effete* in its transit through the foetal system. What becomes of this effete foetal matter? Clearly there is no outlet for it but *through* the mother's system, and by *her* excretory organs.

Judging from the analogy of the process of aeration in the adult, and from the condition of the foetus, which renders all excretion by its own organs (except of bile into the intestine) impossible, carbonic acid and the elements of urine probably form the chief part of that effete matter. Perhaps some *animal* matter may attach to it,—such as is thrown off with the watery vapour in the adult, and amounts, according to Collard de Martigny, to about 3 parts in 1000 of the vapour. This animal matter, however, may be thought too inconsiderable in amount or not of a nature to exert any influence on the maternal constitu-

Mr M'Gillivray brings forward this exsanguine foetal calf to depone in opposition to these exclusive views, and by its testimony to prove that of the blood which returns to the placenta, a *portion* is *absorbed* by the uterine veins, and re-enters the circulation of the mother. That it furnishes "undeniable" evidence of this (as he alleges) is more than can fairly be affirmed, because it is quite conceivable that, without giving back to the mother a single drop of its blood, the foetus may, during the three days, have simply used up the blood within it. Whether this happened it is difficult to say. But it may be questioned whether the vital actions of the foetus would not have failed long before its blood was actually expended to this extent; and it seems altogether much more probable that the nearly complete disappearance of that fluid was owing *in part* to its being abstracted by the mother. That, under the circumstances in which the mother's system was placed during the continuance of the hemorrhage, a powerfully *derivative* effect had been produced on the general mass of her blood, withdrawing a large part of it from the several organs (including the maternal portion of the placenta) towards the seat of the hemorrhage, seems very certain from the observations of Haller and Spallanzani; and that her general function of absorption had been unusually active, the well-known experiments of Magendie sufficiently demonstrate. And we should scarcely conceive, *à priori*, either that the blood of the foetus had not been brought within the sphere of action of those two influences, and, unless an obstacle interposed, been laid under contribution for the support of the mother's system; or that the double layer of nucleated cells, intervening between the maternal and foetal vessels, and which habitually allows the passage of fluids from the mother to the foetus, had then offered any impediment to their passage in the opposite direction.

The discrepancy between Mr M'Gillivray's case, and the statements of Magendie and Williams, as to the *fulness* of the foetal vessels when a pregnant animal dies of hemorrhage, is probably only

tion. But no one, probably, will think so, who reflects on what is now admitted in regard to blood-diseases, and the small influences by which the whole mass of blood may be affected.—(See Kirke's *Physiology*, pp. 71, 286, 292.) The "materies morbi" of scrofula, of syphilis, and of many chronic diseases, must often exist in the blood in an impalpable form,—nay, must even be evolved in the secretions and the plastic exudations. More curious still, even *mind* itself, *immaterial* as it is, must pervade the blood (though its acknowledged seat is the nervous system), and must impart itself to the semen and the microscopic ovum! "In the most perfect animals (says Müller), and even in man, we must suppose that the ovum and semen contain within themselves all the conditions necessary for the production of a new being endowed with (life and) mind; and, consequently, that one or both of them contain (the vital principle and) the *essence* of mind in a *latent* state."—(*Physiology*, vol. i., p. 820.)

Is it more extraordinary, or at all fanciful, to suppose, that a subtle *materies foetus* may attach to the effete matters which pass into the blood of the mother, and that it may *inoculate* her system with its own *distinctive* qualities?

an apparent one. Those statements have reference to cases where the mother dies *suddenly* from *profuse* and *rapid* loss of blood, and where the emptying of the foetal vessels in the only way in which they can be drained, viz., by absorption, is *anticipated by death*; while in Mr M'Gillivray's case, the hemorrhage being comparatively gradual, *time was given* for the exercise of that process, and circumstances concurred to render it *unusually active*.

In the absence of any *experimentum crucis*, whereby a positive solution of the question might be had, the anæmic condition of the foetal calf may, I think, with great probability be ascribed, in part, to the expenditure of its blood in the nutritive processes going on within it, and in part, as Mr M'Gillivray supposes, to the re-absorption at least of its more fluid constituents into the system of the mother. And what seems to have been thus possible, and, under the circumstances, may very fairly be presumed to have actually occurred, and to have taken place pretty rapidly, may reasonably be held to imply, that, at all times, though ordinarily but slowly, there goes on within the gravid uterus a gradual removal by absorption, and reception into the maternal system, of such portions of the foetal blood as are unfitted by reason of impurity for ministering to the nourishment of the foetus, and for the *renovation* of which the placenta may be inadequate. It is not probable, indeed, that the amount returned is very great, and the experiments of Magendie are sufficient to demonstrate that it can be but small. If, however, it take place at all, be the quantity absorbed within a given time never so inconsiderable, it is all that is necessary for the support of Mr M'Gillivray's theory,—the essential principle of which cannot, perhaps, be better expressed than in the words applied to signify the influence of moral causes on human character—“a little leaven leaveneth the whole lump.”

Wagner, indeed, expresses a decided opinion that a *mutual interchange* of fluids takes place in the placenta,—“*the blood of the mother abstracting matter from that of the foetus, and the blood of the foetus taking in its turn matter from that of the mother.*”¹ Müller, also, is of the same opinion. “In mammalia, the vascular villi of the foetus are received into the vascular sheaths of the uterine placenta, so that the capillaries of the foetal and those of the maternal system come into contact with each other, *and suffer an interchange of the matters which they contain.*”² Neither Müller nor Wagner, however, refer to any facts in support of their opinion as regards the absorbent action of the mother on the foetus. Probably they rest it on analogical grounds. And, indeed, all that seems necessary to

¹ Elements of Physiology, translated by Willis, p. 202.

² Elements of Physiology, translated by Baly, vol. ii. pp. 1604—5.—Dr John Reid makes an observation which seems to convey the very same idea:—“The blood of the mother contained in the placental sac, and the blood of the foetus contained in the umbilical vessels, can readily *act and react upon each other.*” —*Physiological, Anatomical, and Pathological Researches*, p. 326.

give confirmation to it, is proof that poisons introduced into the umbilical arteries, or into the body of the foetus, may pass into and affect the mother. That Magendie's experiments failed to show this, is well known; but, valuable as they may be allowed to be, they do not demonstrate that such absorption may not take place,—still less, that it is impossible. The only negative inference that can be drawn from them is, that within the time occupied by them, and under the circumstances in which they were performed, no obvious absorption occurred. And it is conceivable that they may have been too few in number, or too little varied,—nay, even too unskillfully conducted,—to be of much value, and may demand repetition; or, that the circumstances under which experiments of this kind must necessarily be made, are such as to preclude the hope of our ever obtaining satisfactory information through this channel. Yet, even in this event, we may yet obtain the requisite satisfaction. Possibly, nature may herself yet furnish us, in her own way, with the evidence which we cannot extract from her by artificial methods of interrogation. Had we, *e. g.*, undoubted instances of the foetus being tainted with syphilis through the father, and communicating the virus to the mother, we should have all the proof we need desire that matters can and do pass by absorption from the foetus to the mother. Whether such evidence shall yet be had, remains to be seen; but it may be observed that Mr Paget has suggested the criterion by which the value of alleged instances of this kind may be determined.

(5.) It will be necessary to defer, till after consideration of the next division of the subject, the several suggestions to be offered as to the conducting of experiments in breeding on the lower animals, with the view both of testing the principle of *inoculation*, as held and applied by Mr M'Gillivray, and, if a sound one, of giving greater breadth and precision to our knowledge of it. Meanwhile, assuming provisionally that it is a sound one, I would venture to submit, in illustration of it, whether, as suggested to me by Dr Carpenter, the agency of the foetus on the system of the mother is not *dynamical*, rather than material or organic,—operating, in the first instance, on the vitality of the maternal blood, and through it on the vital powers and susceptibilities of the maternal solids? Such, I apprehend, is all that can be imagined of the influence exerted on the living human body by vaccination, or by an attack of small-pox, measles, scarlatina, &c., whereby an almost perfect security is given against the agency of their specific causes in all time coming,—in explaining the *modus operandi* of all or any of which influences, it may be confidently affirmed that the test-tube, the balance, and the microscope, will afford us no assistance whatever.

II. That mental causes, or states of mind, operating within the female during pregnancy, or within either parent at the time of

coitus, may variously influence the nutrition and development of the foetus, has long been matter of popular belief; and, setting a goodly number of recorded instances to the account of old wives' fables, this belief may be allowed to have a stable foundation in facts.¹ It is not my intention, however, to enter on the general subject farther than as it bears on Mr M'Gillivray's theory, referring merely to such facts as serve to show, that mental causes may so influence the growth of the foetus as to produce results analogous to those ascribed to inoculation by a former foetus, and therefore to exhibit a source of fallacy in the reference of these to such inoculation as their cause.

(1.) Of the cases of this kind now to be noticed, some involve changes both in the configuration and the colour of the progeny,—others, so far as appears, changes in the colour only or chiefly.

In Daniel's "Rural Sports" the following details are given respecting the setter-bitch and cur-dog formerly referred to:—

"As the late Dr Hugh Smith was travelling from Midhurst into Hampshire, the dogs, as usual in country places, ran out barking as he was passing through the village, and amongst them he observed a little ugly cur, that was particularly eager to ingratiate himself with a setter-bitch that accompanied him. Whilst stopping to water his horse, the doctor remarked how amorous the cur was, and how courteous the setter seemed to her admirer. Provoked to see a creature of *Dido*'s high blood so obsequious to such mean addresses, the doctor drew one of his pistols, and shot the cur. He then had the bitch carried on horseback for several miles. From that day the setter lost her appetite, ate little or nothing, had no inclination to go abroad with her master, or to attend his call; but seemed to pine like a creature in love, and express sensible concern at the loss of her gallant. Partridge season came, but *Dido* had no nose. Sometime after, she was coupled with a setter of great excellence, which, with no small difficulty, had been procured to have a breed from, and all the caution that even the doctor himself could take was strongly exerted, that the whelps might be pure and unmixed. Yet not a puppy did *Dido* bring forth but was the *exact picture* and *colour* of the cur that had so many months before been destroyed. The doctor fumed, and, had he not personally paid such attention to preserve the intercourse uncontaminated, would have suspected that some negligence had occasioned his disappointment; but his views were in many subsequent litters also defeated, for *Dido* never produced a whelp which was not exactly similar to the unfortunate cur, who was her first and murdered lover."²

In Mr Blaine's "Encyclopædia of Rural Sports" this other case is given:—

"The late Lord Rivers [says Mr Blaine] was famed for a breed of black-and-white *spaniels*, one of which, having more than the usual quantity of white, he presented to us. We had, at the same time, a *pug-bitch* of great beauty. The attachment of this bitch to the spaniel was singularly strong. When it

¹ Any one curious in cases of this kind may advantageously consult Dr Allen Thomson's article on "Generation," in the Cyc. of Anat. and Physiology; and also an article in the Ed. Med. and Surg. Journal, vol. xxv., p. 134.

² Daniel's Rural Sports, vol. iii., pp. 333, 334.—A case very similar to the above, occurring in a bitch belonging to him, has been mentioned to me by Mr Walker, Portlethen, in Kincardineshire.

became necessary to separate her, on account of her heat, from this dog, and to confine her with one of her own kind, she pined excessively; and, notwithstanding her situation, it was some time before she would admit the attentions of the pug-dog placed with her. At length, however, she was warded by him, impregnation followed, and at the usual period she brought forth five pug-puppies, *one of which was perfectly white, and rather more slender than the others, though a genuine pug.* The spaniel was soon afterwards given away. At two subsequent litters (which were all she afterwards had) this bitch also brought forth a *white* pug-pup, which the fanciers know to be a very rare occurrence. It is also a curious fact, that each succeeding white puppy was *less slender* in form than the preceding, though all were 'equally white.'¹

The two cases now given have many points in common, and appear to be free from any material source of fallacy. In the former there was not even sexual intercourse—much less fruitful intercourse—between the bitch and the cur, to whom her progeny bore so decided a resemblance; and in the latter, if intercourse occurred, which it appears did not, there was no result from it. In both females there seems to have been a strong and an abiding attachment to the cur and spaniel respectively, and an equally keen and enduring emotion of disappointment at being separated from them. The resemblance of the progeny, or of certain of these, to the dogs, appears to have been of too special a kind to admit the supposition of its being accidental. There need be no question, therefore, but that the cases are fair examples of mental impressions of a permanent character so operating on the female parents as to influence the development of their offspring while in *utero*, and that in such a manner as to cause them to resemble the male animals that were the objective causes of those impressions.

Nearly similar remarks apply to the following case, for the particulars of which I am indebted to Dr John R. Trail, of Monymusk, in Aberdeenshire. The chief point of interest in this case lies in the resemblance being at once general, and yet extending to an abnormal peculiarity of conformation, and in its thus forming a connecting link between the two foregoing cases and two others presently to be adduced. It does not appear, indeed, whether the mother manifested any special attachment to the male animal which her offspring resembled, but such attachment may reasonably be presumed:—

A mare and a horse (a gelding), belonging to a friend of Dr Trail, had for some years worked together on the same farm, occupied adjacent stalls in the same stable, and pastured together in summer in the same fields. The gelding was of a black colour, with white legs and face, and had a singular peculiarity in the form of the hind legs, which, when the animal was standing, appeared as if quite straight, there being no appearance of the leg being bent at the hough-joint, as in ordinary cases; the pasterns likewise were very long, so as to cause the feet to look as if placed almost at right angles to the legs.

Having been some years thus associated with this gelding, the mare was covered by a stallion of the same colour with herself,—both stallion and mare being of a bay colour, with black legs, and a small spot of white only on the

¹ Encyclopædia of Rural Sports, by Delabere P. Blaine, Esq., p. 412.

forehead. The foal which was the produce of this connection very exactly resembled the gelding in *colour*, and in the *shape*, too, more particularly of the hind legs, as above described. "From the description I have attempted to give you [Dr Trail writes me], you could not form any very distinct idea of the peculiar conformation of the horse; but the resemblance of the foal to him was remarkably clear."

Dr Montgomery, of Dublin, gives the following interesting case:—

"A lady, pregnant for the first time, to whom I recommended frequent exercise in the open air, declined going out as often as was thought necessary, assigning as her reason, that she was afraid of a man, whose appearance had greatly shocked and disgusted her: he used to crawl along the flag-way, on his hands and knees, with his feet turned up behind him, which latter were malformed and imperfect, appearing as if they had been cut off at the instep, —and he exhibited them thus, and uncovered, in order to excite commiseration. I afterwards attended this lady in her lying-in; and her child, which was born a month before its time, and lived but a few minutes, although in every other respect perfect, *had the feet malformed and defective, precisely in the same way as those of the cripple* who had alarmed her, and whom I had often seen."¹

Nothing can be more appropriate than Dr Montgomery's short commentary on this case:—"Here was an obvious and recognised object making a powerful impression, of a disagreeable kind, complained of at the time, and followed by an effect in perfect accordance with the previous cause,—there being between the two a similarity so perfect, that, with Morgagni, I 'will not easily suppose that chance could have been so ingenious, if I may be allowed to speak thus, and so exact an imitator.'

A case very similar to the foregoing has been communicated to me by a distinguished English physiologist,—himself personally cognisant of the facts. It is as follows:—

"A lady, when two or three months pregnant, was accosted by a *one-armed* beggar, who, on her refusing to relieve him, menaced her violently, so as to alarm her seriously, and shook his stump at her. She was extremely agitated, and during the whole remainder of her pregnancy was under the firm expectation that her child would be *one-armed*,—which was the case. This child is now grown up to manhood, and occupies a highly respectable position in society."

In this case (and the remark applies equally to Dr Montgomery's) the effect was *partial* only, and on this account it may be thought not altogether pertinent to the subject under consideration; but it probably embraced all that was peculiar in the object, as it certainly did all that was striking to the mind of the lady. And had the occasion of the impression been some well-marked but normal peculiarity of the features,—*e.g.*, of the nose,—instead of a deformity of the arm; and had the man been the object formerly of the lady's love and attachment, and still during her pregnancy of her cherished regard,—and had the resemblance in her child extended to the part supposed,—the effect, though equally partial, might have seemed more general. The case might thus have been set down as a fair example of the power of the imagination, in a pregnant female, to cause her off-

¹ Exposition of the Signs and Symptoms of Pregnancy, &c., pp. 16, 17.

spring very exactly to resemble an individual not its father. Of this the following is, perhaps, an instance:—

A young married woman, residing in Aberdeen, between whom and a young man a strong attachment and a matrimonial engagement had long existed, but who were never married, and never had sexual intercourse together, gave birth to a child, which bore so striking a resemblance in its features to the woman's first lover, as to attract the notice of herself and many others of the acquaintance of the parties.

In this case—communicated to me by Mr Robert M. Erskine, surgeon, here, who was well acquainted with the individuals concerned, and had personally satisfied himself of the accuracy of the facts—the resemblance may have existed only in the imagination of the observers, or been magnified through the love of the marvellous,—and, giving it as given to myself, I adduce it merely as a possible example of what may be a real occurrence, and would contrast remarkably with the observation alluded to by Dr Allen Thomson,—viz., “that the human female, when twice married, bears occasionally to the second husband children resembling the first, both in bodily structure and mental powers,”—and also with the cases given as instances of this, in the Appendix to my former paper.

It appears that many breeders of stock are impressed with the belief, that certain *colours* present to the eye of the parent animals, and particularly of the female, at the time and in the act of their being coupled together,—and to the eye of the female both before and during her pregnancy, influence the *colour* of the progeny; and that they make this belief a practical principle of action in the breeding of their stock, in order either to prevent or to secure the admixture of any particular colour in the offspring different from that of the parent animals. “We know,” says an anonymous writer, “a great breeder of pure Angus stock [black polled breed], who makes it a rule to have every animal about his farm of a black colour, down to the very poultry.”¹ And an eminent breeder of the same stock in this county informs me, that he extends this rule to the *steadings* in which his cattle are kept.

To illustrate generally the grounds of this belief and practice, the following cases may be cited:—

(a) A black polled [Angus] cow, belonging to Mr Mustard, a farmer in Forfarshire, came into season while pasturing in a field bounded by that of a neighbouring farmer. Out of this last there jumped into the other field an *ox*, of a *white colour*, with black spots, and *horned*, which went with the cow till she was brought to the bull,—an animal of the same colour and breed as herself. Mr Mustard had not a horned animal in his possession, nor any with the least white on it; and yet the produce of this (black and polled) cow and bull was a black-and-white calf, with *horns*.²

¹ “North of Scotland Gazette” newspaper, for July 17, 1849.

² Library of Society of Useful Knowledge, volume on Cattle, p. 171.—This seems to be the same case as that given by Dr Allen Thomson, in Cyc. Anat. and Phys., art. “Generation,” vol. ii., p. 474.

(b) Last year (1849), twenty cows of the black polled Angus breed,—belonging to Mr William M'Combie, in this county, and whose stock is perhaps the finest in the kingdom,—produced as many calves, all of them black and polled, except one single calf, which was *yellow-and-white spotted*. Mr M'Combie had, as usual with him, taken the precaution of causing the cows, both before and during their pregnancy, to mix with none save perfectly black cattle, except in respect of the mother of this calf, which cow had unwittingly been put to an out-farm, to be starved, in order to fit her for the bull. There, for a considerable period prior to her being served with the bull, she had grazed with a large *yellow-and-white spotted ox*, of which ox the calf she subsequently bore was the very picture,—the likeness, however, extending no farther than to the colour, and the calf still retaining the shape and configuration of its parents, which were both of the same breed and colour.

(c) Out of a large herd of cows, of the pure Teeswater breed, all of them of the brown or roan colour (belonging to Mr Cruickshank, Sittyton, near Aberdeen), there is every year dropt one, or at most two, *white* calves, which, in order to prevent the introduction of this colour among the cattle, are invariably sold, and sent away. Last year, however, concurrently with the *white-washing* of all the farm-steadings, the very large number of twelve white calves were produced. And the like occurrence happened last year also, in the herd of an extensive breeder of the same kind of stock, in Yorkshire, in connection with the like process of white-washing,—this process having, in both cases, been very extensively carried out before the breeding season began, with the view of preventing the breaking out of the pleuro-pneumonia, then epidemic in the neighbourhood, and very destructive.¹

(d) "At the time when a stallion was about to cover a mare, the stallion's pale colour was objected to, whereupon the groom, knowing the effect of colour upon horses' imaginations, presented before the stallion a mare, of a pleasing colour, which had the desired effect of determining a dark colour in the offspring. This is said to have been repeated with success in the same horse more than once."²

(e) "I was told (Mr M'Combie writes me) by an old servant of mine, Morrice Smith, that, when he was a servant in the parish of Glass (Aberdeenshire), a black bull served a black cow at the time when a white mare passed them, and that the produce was twin *white* calves. There were no white cattle upon the farm where this occurrence happened."³

Such cases as several of those now cited can scarcely fail to recall to the reader's mind the story, given in the book of Genesis (chap. xxx.), of Jacob and his peeled rods, and the effect of these in causing

¹ Communicated by Mr Cruickshank, who informs me further, that he has had too many proofs of the agency of the cause in question to allow him entertaining any doubt on the subject.

² Art. "Generation," in Cyc. Anat. and Phys., vol. ii., p. 474.

³ My friend Dr J. M. Duncan writes me, that he has "more than once heard farm-servants say, that it is a sure plan to get a white foal, to hang up a pure white sheet before the mare when she conceives." Probably hanging up such a sheet in the stable during the whole period of pregnancy would be equally effectual. And another correspondent (J. Warwick, Esq., Sussex House, Hammersmith) says: "The belief in the effect of external objects on the imagination of pregnant women is so strong in Italy, that females in that condition keep in their rooms, and before their eyes, small wax figures of a *pretty child*. This is called 'Il Bambino,' represents the infant *Jesus*, and is especially worshipped and adored, with the view of procuring beautiful offspring."

the flocks, before whom they were placed at the time of conception, to bring forth ring-straked, speckled, and spotted cattle.

It does not appear from the sacred narrative whether the influence of the rods was exerted on the minds both of the male and female cattle, or confined to those of the female. But it seems probable (Gen. xxxi., 5-12) that the effect was a *supernatural* one, and designed to enrich the needy patriarch at the expense of the crafty Laban, by whom, for fourteen years, he had been sore let and hindered "in providing for his own house;" and that Jacob had beforehand, in a dream, intimation given him of the design, as well as a sensible representation both of the agency to be employed and of the result which was to follow.¹ If this were the character of the transaction, it would be unwarrantable to draw any inference from it in relation to the present inquiry, unless it could be shown, which I apprehend it cannot, that the same expedient will now, as then, produce the like results. At the same time it may, perhaps, be not unfairly referred to, in illustration of ordinary phenomena of a somewhat analogous kind. Read by the light of these, it may be held to indicate that the Almighty accomplished His purpose simply by *enhancing* a natural agency—"moulding it secretly in the hollow of His hand,"—and so be regarded as reflecting back light in its turn upon that agency. The validity, however, of this appeal must turn upon our being able to satisfy ourselves, on independent grounds, that there is in nature an agency of this kind, and its precise value upon the extent to which we can thus ascertain that it ordinarily operates,—questions these for the solution of which we have not at present the requisite data.

(2.) All that need be said in the way of direct inference from the facts brought together in this division of the subject, may be comprised within a narrow compass.

Supposing the statements respecting them to be authentic—and no question, I apprehend, as to this can well be raised—the cases are

¹ Dr Allen Thomson, in his comment on this transaction (Cyc. Anat. and Phys., art. "Generation"), seems to me to have very completely missed the import of the essential parts of the narrative. He gathers from it, that Jacob had parti-coloured males to breed with, and makes the result an affair of mere hereditary transuission. But Jacob had not this advantage, and it seems clearly to have been part of his own proposal to Laban that he should not. Laban, at least, on the bargain being concluded, very carefully singled out all the animals of that sort,—males as well as females,—even "to every one that had some white on it, and all the brown among the sheep;" and, giving them into the hand of his sons, set three days' journey betwixt this parti-coloured flock and that which was to be tended by Jacob. A difficulty, it may be added, attaches to the right understanding of the whole matter, from the obscurity of v. 40 (chap. xxx.)—the true meaning, if not the proper rendering, of which appears to be this:—"And Jacob did separate the lambs, and set aside from the flock all the ring-straked and all the brown in the flock of Laban; and set them apart for a flock to himself, and put them not unto Laban's cattle."

nearly unequivocal. The only fallacy that can attach to them, is that arising from the possibility, that the peculiarities in the progeny were either purely *accidental*, or owing to corresponding qualities *latent* in the parents, but breaking out in the offspring. The relation, however, in most of the cases between the peculiarities in question and their presumed causes, is too close and of too special a character to admit of either supposition. We are, therefore, well entitled, I think, to regard the greater number, if not the whole of them, as examples of mental causes so operating either on the mind of the female and so acting on her reproductive powers, or on the mind of the male parent and so influencing the qualities of his semen, as to modify the nutrition and development of the offspring.

How, in respect of the female, this influence is exerted,—what is its *modus operandi*,—and what the conditions of its agency, it is not easy to determine. The mental affections seem to have been in most of the cases, and were probably in all of them, of a *strong* and *enduring* kind; and we can easily conceive this to have been essential to the result. That the alteration in the growth of the foetus was determined solely, as is vulgarly supposed, by the *images* in the mind of the mother,—*i.e.*, by the mere *sensations* and *perceptions* therein produced, independently of the *emotions* excited by them,—cannot well be supposed. It is, doubtless, to this “*compound state*” of mind—to use an expression, without adopting, however, the psychological theory, of Sir James Mackintosh—“easily called to mind,” in consequence of the vividness of its first impression, “frequently recurring,” and “warmly felt,”¹ that we must ascribe the effect.

It is not unlikely that this particular agency of the mind is more frequently exerted in the females of the lower animals than in those of our own species; and that cases exemplifying it are oftener met with in the brutes than in man. If this be so, a reasonable explanation of the fact may be given. We know that the minds of the lower animals are in a great measure limited to *particulars*, and these few in number, and almost exclusively external objects of sense; that the external senses are more perfect in them than in us; and that the perceptions resulting from their exercise seem, in various instances, to follow more surely and more quickly—to be more intuitive and wider in their scope, and more vivid—in them than in man; and that the simpler emotions (excited by those perceptions) of joy, fear, affection, anger, &c., of which they are manifestly susceptible, seem often to be peculiarly strong.² We know also that they possess the faculty of *memory*; and we may well suppose, from their limited range of association (or suggestion), that sensations that formerly made a powerful impression on their minds, will be more easily and oftener recalled in them than in us, who, though more apt to be “troubled about many things,” are proportionally less apt to be

¹ Ethical Philosophy, edited by Whewell, 2d edition. pp. 397, 398.

² See art. “Instinct,” in Cyc. of Anat. and Phys.

affected, or at least permanently or continuously impressed, by any one thing. These circumstances and peculiarities of mental action must obviously be singularly favourable to the production of the results in question,—keeping in mind, that the mental agency under consideration is manifestly closely connected with *sensations* and with simple *perceptions* and *emotions* thence resulting, and with a certain *intensity* and *endurance* of all these.¹

That the parts of the female system specially affected by this “compound state” of mind are the blood and nutritive processes generally seems very probable; that the *kind* of change, however, therein produced, is rather *dynamical* than organic, has (just as in regard to the *inoculation* principle) been suggested to me by Dr Carpenter; that it may be of identically the same nature with the change produced by the inoculation principle is quite possible, the two, however different in their origin, thus running up into, and meeting in, one *common* principle,—to wit, a modification of the constitutional or dynamical powers of the female; and that it may be more or less *persistent*, and operate on a subsequent conception, independently of a renewal of the mental state itself, some of the cases seem to indicate. That the change in question is produced through the medium of the nervous system, we cannot doubt—“that being the acknowledged seat and instrument of mental acts;” and that the parts of the nervous system more immediately concerned, in addition to cer-

¹ The word *imagination* generally used to designate the mental states here concerned, is an unfortunate one; and it may be questioned whether much of the scepticism prevalent among physiologists as to the effects, or alleged effects, on the development of the foetus, of certain complex mental affections, is not in great part owing to the use of that term; and whether, also, we have not here an illustration of the observation—“*homines putant rationem suam verbis imperare, sed fit etiam ut verba vim suam super intellectum retorqueant.*”

That certain mental *conceptions* or *ideas*, suggested to the mind independently of any present or actual external object, may, if they excite strong and enduring *emotions*, influence the growth of the foetus, seems very probable; and cases of this kind are on record. But this, which is essentially a “compound” state of mind, cannot be called a mere act or state of *imagination*. In general, however, the state of mind with which the emotion is associated, in these cases, is one of *perception*, excited through the external senses, and into which the *imagination*, in any sense of the term, does not enter as an element. It is one compounded of *sensation*, *perception*, *emotion*.

The part which each of these plays in the production of the physical result on the foetus, it is not easy to determine. No doubt, the *direction* (so to speak) in which the influence of the emotion is exerted, is dependent on the perception, or the idea, which excites it—otherwise no correspondence would be observed between the effect on the foetus and the objective cause of the perception, or the subject of the idea,—and the effect, when produced, would be indeterminate in its character, *e.g.*, the sight of a bright yellow colour might give rise to a purple one in the foetus, or the idea of the child to be born having a tail to its being a cyclops. Still, in order to the production of the effect, it seems almost, if not quite essential, that the perception or the idea should be *backed*, if the expression may be allowed, by an emotion of a strong and an abiding kind. And if so, the effect altogether must be ascribed to a *complex* or *compound* state of mind,—a state, however, neither necessarily nor peculiarly one of *imagination*.

tain great nervous centres (the sensory and emotional ganglia¹), are the ganglionic nerves, may reasonably be conjectured—these nerves being the only channels by which the blood and the nutritive processes can be brought into relation with the mind.²

Beyond this we cannot venture to go in the way of speculation. The singular influence thus exerted by the mind of the mother on the growth of the foetus, is not one “for which,” as has been remarked of other modes of action of the mind on the body, “it is likely that we shall ever be able to assign a reason, or which it would be any great hardship to be obliged to regard as an ultimate fact in physiology.”³

III. Reverting now to the theory of inoculation, and to the cases cited as instances of it, it will be obvious, from the facts brought together under the preceding head, that in any ordinary case where an animal resembles a male, not its progenitor, by which its mother had on a former occasion been impregnated, the resemblance may be explained as well on the principle of mental agency as on that of inoculation; or, at least, that in ascribing it to this latter cause a manifest source of fallacy attaches to the assumption.

To obviate this, and to determine conclusively whether or not the phenomenon is independent of mental agency, all that seems necessary is, to institute experiments in breeding on a sufficiently large number of different kinds of animals, selecting animals of acknowledged purity of blood, and conducting the experiments so as to exclude (which I understand could easily be done) the agency of any such mental impressions on the minds of the animals concerned, as could reasonably be supposed to influence the results. Should these be of an affirmative kind, and be, at the same time, sufficiently numerous and decided, it would follow conclusively that the phenomenon is not due to mental causes. In connection, however, with this set of experiments, it would be desirable to institute another set in which every advantage should be given to the agency of mental causes. Should the results from these be equally affirmative—although this circumstance would not vitiate the inference fairly deducible from those of the other set, unless by exhibiting in these any source of fallacy otherwise perhaps imperceptible—they would afford points of comparison and contrast, and thereby enable us the better to appreciate the value of that inference; while, if they should be wholly or in a great measure negative, it is clear that they would add greatly to the conclusiveness of the inference in question.

¹ See Carpenter's Manual of Physiology, p. 522, et seq.; and Todd and Bowman's Physiological Anatomy, &c., vol. i., chap. xi.

² Alison—Outlines of Physiology, 3d edition, pp. 402-4; and Physiological Inferences from the Study of the Nerves of the Eye-ball, in “Medical Gazette,” No. 705 (June 1841), p. 410, et seq.

³ Alison—On the Physiological Principle of Sympathy,—Transactions of Edinburgh Medico-Chirurgical Society, vol. ii., pp. 223, 224.

The following is a detailed outline of the most important of these
EXPERIMENTS.

First Set.—To determine how far the circumstance of a previous impregnation by a male of a different species, breed, or colour, from the female—the agency of mental causes on the result being excluded— influences the conformation or colour of the offspring subsequently born by her, to a male of the same breed and colour as her own :—

The female to be served the first season by a male of a different species, breed, or colour from her own ; and the following season, by a male of the same breed and colour as herself,—taking these precautions against the agency of mental causes, *first*, and generally, that the female shall not at any time, up to the period of her second *accouchement*, have seen the former of those two males, nor any animal of that species, breed, or colour ; and, *secondly*, and particularly, that the coitus between her and that male shall be so managed as to prevent her from seeing him ; that, subsequently, during this her first pregnancy, the female shall be kept exclusively with animals of her own breed and colour ; and that, immediately on her delivery, the (cross) animal produced be taken from her, and ever after kept out of her sight.

Second Set.—To determine how far mental causes, operating through the external senses, influence the colour and conformation of offspring :—

I. Mental causes operating at the time of coitus only, or at least originating then—

The male and the female, both of the same breed and colour, in being coupled together, to have fully in their view animals of a different species, breed, or colour, or objects of any kind of a different colour, regard being had, in respect of the matter of colour, that this present a marked contrast to that of the animals concerned, and, particularly, that it be of a bright or striking hue.

To prevent, as far as possible, any influence of this kind from operating on the mind of the female during her pregnancy, the female to be made to herd exclusively, till after her delivery, with animals of the same breed and colour with herself.

II. Mental causes operating on the mind of the female during pregnancy only—

The male and the female, both of the same breed and colour as before, to be coupled together in the usual manner (or in the presence of animals of the same sort only) ; but the female, during her pregnancy, to herd exclusively with animals of a different species, breed, or colour.

III. Mental causes operating both at the time of coitus, and subsequently during the pregnancy of the female—

The male and the female, in being coupled together, to have fully in their eye animals of a different species, breed, or colour ; and the female, during her pregnancy, to herd exclusively with animals of this sort.

The details now given embrace the principles on which, as it seems to me, decisive experiments might be made, and also the precautions necessary to be attended to, in order, as far as possible, to avoid fallacy in the results obtained from them. They might be very variously modified, and ought certainly to be tried on many different

species of animals, in order to give at once a breadth and precision to the results.

Should it be clearly established by such experiments that an animal, the produce of a male and female of the same breed, born subsequently to the impregnation of the female at a former period by a male of a different breed or species from her own, has, from the single circumstance of such previous impregnation of its mother, its nutrition and development so far changed as to resemble that male animal, not its progenitor, the experiments would farther supply materials towards determining the extent to which the fact holds, and the conditions essential to its occurrence. And, in particular, they would furnish more or less satisfactory answers to the following questions,—themselves suggestive, both of modifications in the experiments, and of the points to be attended to in noting the results—

1. Does the fact hold *universally*, or only *pretty often*, or only *occasionally* and *rarely*?
2. Does the animal so produced partake *decidedly* or only *partially* of the characters of the male animal which it resembles,—in *colour* merely, or in *conformation* also?
3. Does the degree of resemblance depend on the number of times the mother may have conceived by that male, before bearing the animal in question?
4. Is the resemblance lessened or in anywise affected by the female having been *first* impregnated by a male of her own breed and colour, prior to her having born offspring by that other male?
5. Does the resemblance to that male become less and less in each animal successively produced, and does it ultimately become imperceptible?

Whether such experiments as are here suggested would, if instituted, establish as a principle in the physiology of generation, that, independently of mental agency, a male animal, that has once had fruitful intercourse with a female, may influence the products of subsequent conceptions of that female in which he himself is not concerned, it were needless at present to speculate. But the cases given in this and my former paper, as instances of it, taken along with the belief of the frequent occurrence of such cases, and the acknowledged importance of maintaining the purity of breed of most kinds of our domestic animals, may well engage the attention of the agricultural body, and lead them to investigate the subject. How much the Royal Agricultural Societies of England and of Ireland, and the Highland and Agricultural Society of Scotland, have it in their power to promote such an investigation; and how much the prosecution of it would be in keeping with the ends of their institution, it is unnecessary to say.

IV. The experiments suggested under the preceding head have regard mainly to the question, whether the plienomenon, of which an explanation is sought, may not be adequately accounted for on the principle of *mental* agency. It is conceivable that they may

demonstrate that it is independent of any such agency, and referable, therefore, to a *material* agency connected in some way with the prior coitus of the female with the foreign male. And should they establish this, an important step in the inquiry as to its real cause would be gained. They cannot reasonably be expected, however, to demonstrate, at least directly or necessarily, that it is due to a *constitutional* change effected in the system of the mother by the foetus begotten by that male. For, in fact, the cause of it may be, as is supposed by some, a strictly *local* change confined to the general mass of ova (or to certain of these), and effected simply by the *semen* of this male. Nay, as a professional friend has pointed out to me, even supposing the change to be a constitutional one, it may not be produced by a foetus in utero, but by the *semen* received by absorption into the female system, just as the small-pox virus, and other morbid poisons, are thus taken into the system, and produce in it a general and permanent change of some kind.

It is not my intention to enter into a discussion here on either of these two other explanations of the phenomenon. The first, indeed, was pretty fully considered in my former paper; and I have only farther to submit, in reference to it, that if it should appear that *impregnation* at the time of the former coitus is a necessary condition of the phenomenon, the theory of a local change, effected merely by the semen, will thereby be rendered an improbable one; because it is certain that, in the human species at least, the semen must often reach the ovaries without impregnation following (and this just because of no ova being ripe); and it seems difficult to comprehend how, if the change is of the kind supposed, and referable to some action of the semen on *immature* and then *unimpregnated* ova (and this theory necessarily implies as much), the phenomenon should occur only when that condition holds.

With regard to the other explanation, it may be conceded in support of it, and as showing that absorption of the liquor seminis is not an unlikely occurrence, that secondary syphilis may be imparted by a man to a woman through the medium of that fluid, because imparted independently of impregnation. But whether the syphilitic virus is often conveyed in that way, or more generally only when impregnation follows intercourse, remains to be seen (see p. 7). In the meantime, accepting the suggestion, and registering it here along with the other theories as equally with them furnishing a possible explanation of the phenomenon, I would merely now observe regarding it, that, just as in regard to the theory of a local change, its antecedent value will turn mainly on the circumstance, whether the phenomenon with which it is sought to connect it is ever observed in cases where impregnation has not followed a former coitus. If such impregnation is an essential condition to its occurrence, the explanation in question may be set aside as worthless.

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